

Peer-review report of

Robson, S.G., Faasse, K., Gordon, E.-R., Jones, S. P., Drew, M., & Martire, K. A. (2024). Lazy or different? A quantitative content analysis of how believers and nonbelievers of misinformation reason. *advances.in/psychology*, 2, e003511. <https://doi.org/10.56296/aip00027>

Round 1

Dear Authors,

Thank you for submitting your intriguing and important research to our special issue in *advances.in/psychology*. I had the privilege of receiving evaluations from two reviewers who are experts in the field of the submitted work. I appreciate their exceptionally detailed, constructive, and insightful feedback. As is standard practice, I reviewed and evaluated the manuscript myself before considering the reviewers' comments.

We all recognize the merit in your research. You address a significant topic using an underutilized method in the field and present two studies that appear to replicate the results. This innovative approach underscores the potential of your work. However, we believe that some aspects of your study could be further developed. The reviewers have highlighted several areas where you can strengthen your theoretical background and arguments, and clarify how your tests align with your research question. Additionally, both reviewers suggest improvements to your analytical approach, along with other important points. I will not reiterate their comments here, as they have outlined them in detail and in an actionable manner. However, I ask that you address each of their points and outline your revisions, including excerpts demonstrating the changes, in a response letter.

Below are my own comments, some of which overlap with the reviewers' thoughts, while others are additional observations.

Terminology and Operationalization

I concur that the cutoff used to identify individuals as “fringe” believers was arbitrary, and that the scale should be used as a continuous measure. You can explain this deviation from your preregistration as a request from the reviewers.

Description of Experiments

Please clarify how participants were recruited (e.g., the name of the online panel) and from where (e.g., country).

The description of the experimental design could be improved. The presentation of irrelevant information in Table 1 was unclear. It seems as though these items were included as fillers, yet there is an indication of their intersection with experimental factors. This needs further clarification. For instance, it was unclear why “She is also a candlemaker” was considered high quality and relevant, whereas “she is a pilot in her spare time” was seen as irrelevant. Being a pilot likely influences perceptions of general competence and may be considered relevant to expert perceptions. While this was hinted at in a table footnote, the rationale deserves more explicit attention, and it would be helpful to know if you accounted for this in your analysis.

Please clarify whether you manipulated all ExPEX dimensions in the same way rather than independently (as in a conjoint-type experiment). Also, explain the within-subjects component specifically.

Analyses and Reporting

I understand that the authors recoded justifications into three overarching categories. It would be valuable to present the prevalence of each specific sub-justification (within these overarching categories), broken down by groups and conditions, for instance in a figure.

Please providing a table with qualitative examples of each overarching category and their subcategories. It would also be helpful to know which ExPEX dimensions the justifications were related to, to understand whether different groups paid different attention to different dimensions. Similarly, for unrelated justifications, it would be interesting to know the topics they focused on. In other words, there is a lot of untapped potential in your data.

It seems as if qualitative responses were aggregated to the participant level as count variables. As the reviewers suggest, a Poisson regression should be used to analyze such data. An even more suitable approach may be logistic (Bernoulli) multi-level analyses, where responses (with 0 vs. 1 values) are nested within participants.

A critical point is that the analyses appear to ignore the experimental manipulation of low vs. high quality. This factor should be included to investigate differences in study groups' use of the three categories of justifications. Please provide these additional analyses.

As the reviewers also note, ensure that non-significant effects are described appropriately. Instead of stating “the groups did not differ,” which implies a null-hypothesis test, write “the groups did not statistically significantly differ” or provide appropriate tests (e.g., line 210, and elsewhere).

Minor Points

- There are a few typos here and there. E.g.,
- Please revise the sentence on lines 140-141 (“and including” is not needed).
- There is a missing word on line 155.
- Typo on line 306.
- Provide 95% error bars in figures.
- Ensure each DOI number is provided as a URL in the references, in line with APA7 guidelines.

In summary, while your research has the potential to make significant contributions to the field, it is not yet fully realized. I therefore ask you to revise your manuscript according to the outlined points. Please reach out to me at j.r.kunst@psykologi.uio.no if you require an extension of the word count for this or have other questions.

Best regards,

Jonas R. Kunst,

Editor-in-Chief

Reviewer 1:

Thank you for the opportunity to review this manuscript. The manuscript reports two studies testing whether individuals who hold fringe vs. mainstream beliefs differ in their evaluation of new information. Overall, I enjoyed reading the manuscript and found it to be well-written, with novel and interesting ideas. However, I have several concerns.

First, I think several decisions, at present, lack sufficient justification and contexts. For one, I am not entirely convinced by the classification of participants as “fringe believers” if they rate their endorsement of at least one of the four implausible claims as at least 60. It is also unclear to me what the rationale was for selecting these four specific claims (i.e., climate change, vaccines, moon landing, and flat earth), and

whether endorsement that is slightly higher than the midpoint on one of the items would both (1) indicate that someone truly holds this fringe belief (this measure seems unreliable and prone to error), and (2) would be equivalent to a similar level of endorsement for a different item (e.g., climate change denial vs. moon landing denial are treated as equivalent here). If lack of belief in all four claims was needed to classify a participant as a mainstream believer, why was belief in one claim enough to classify a participant as a fringe believer? Likewise, I'm not sure why fringe and mainstream belief was dichotomised, rather than belief in implausible claims being treated as a continuous predictor. These decisions, unless explicitly justified in a revision, seem somewhat arbitrary and/or broad. Indeed, many participants might endorse one or more of the implausible claims for various reasons (e.g., indicating endorsement for the moon landing conspiracy for humor, or for the claim that climate change is a hoax for ideological reasons). Consequently, the fringe believers in the studies may, in fact, not represent the fringe, and what is being tested would then be an extreme version of the miserly hypothesis (one that isn't actually supported by the literature)—that laziness underlies all false beliefs at all times—rather than the more reasonable version that is proposed in the literature, which suggests that laziness underlies false beliefs under certain conditions, such as under information overload or in the presence of distractor cues (e.g., on social media).

Second, from my perspective, the authors should also provide further justification for not reporting the preregistered primary dependent variables, the rating scales, in this manuscript. Although this is not necessarily problematic, as the authors are fully transparent about this decision (and have provided links to the preregistrations), the results of the open-ended question should ideally be interpreted in light of the results for the rating scales. For example, if all participants, regardless of whether they're in the fringe or mainstream groups, struggle with the task, then it could be that the task is simply challenging for all participants. Thus, capability could be confounded with laziness. The miserly hypothesis presumes that participants can reach the correct conclusions if they pay more attention or deliberate more. If all participants struggle with the task, regardless of whether they're "lazy", then the results do not really provide support for or against the miserly hypothesis. To be clear, I do not think that the authors must rewrite the manuscript to include the rating scales. However, I think more information regarding the overall patterns of results could help readers better contextualize the justifications provided by participants and thus substantially improve the manuscript.

Third, I am not sure if a mixed ANOVA is the most appropriate choice for the analyses. The dependent variable, the number of justifications, is count data, and the number of zeros seems high based on the plots. I encourage the authors to check if the current data has an inflated zero count. If the data does not I would recommend the authors run a Poisson regression, and if there is an inflated count I would recommend they run a zero-inflated Poisson regression (the authors may also benefit from running these as mixed-effect models). In addition, if the authors seek to predict and quantify evidence in favor of a null (i.e., no difference between mainstream and fringe participants in terms of the number of justifications), a Bayesian approach would be more appropriate than the NHST used here.

Finally, the authors interpret the results as supporting the information preference hypothesis and being inconsistent with the miserly hypothesis. Perhaps I'm missing something, but, even assuming perfect measurement and that results hold across modeling approaches, I am quite unsure about this interpretation. The miserly hypothesis for misinformation beliefs in the literature focuses on when individuals are first exposed to false claims. What the reported studies assessed as dependent variables, however, are post-hoc justifications on the part of the participants. To me, there is no reason differences in analytical effort or deliberation must be reflected in the raw number of such post-hoc justifications. For example, couldn't a difference in the type of justifications provided also be a result of a difference in analytical effort and deliberation? From my perspective, one can reasonably argue that it is precisely because the fringe participants did not sufficiently deliberate that they failed to encode (and subsequently report) the quality cues under present-relevant justification. I therefore encourage the authors to cut or tone down statements such as "given the lack of evidence that Fringe and Mainstream believers differ in analytic effort, simply encouraging more analysis may not be effective".

Reviewer 2:

I read this article exploring differences in reasoning styles between individuals who hold epistemically suspect beliefs (i.e., misbeliefs) compared to those who do not hold such beliefs with great interest. The authors build on research showing that analytic thinking tendencies and belief in misinformation correlate and they examine whether individuals who hold misbeliefs reason less or differently compared to individuals who do not hold such beliefs. Across two studies, the authors qualitatively examined individuals' justifications for their reasoning about expert evidence they were presented with and quantitatively compared the extent (i.e., length and number) of justifications as well as their nature between individuals who endorse misbeliefs and individuals who do not. Based on these analyses, the authors suggest that individuals holding misbeliefs do not appear to differ from those who do not in terms of the extent to which they reason about evidence, but they do differ in the nature of their reasoning. More specifically, the authors claim that individuals who hold misbeliefs rely less strongly on information about epistemic authority (i.e., expertise) in their reasoning than individuals who do not hold misbeliefs.

Firstly, I applaud the authors' approach to investigate individuals' reasoning processes in detail by analysing them qualitatively. I believe this approach provides much-needed nuanced insight into such reasoning processes, thereby adding a new perspective to much prior work on this topic that examined related questions by measuring dispositional tendencies related to reasoning processes. I believe that investigations into how the nature of reasoning potentially affects individuals' susceptibility to misbeliefs could substantially advance understanding of the formation of misbeliefs and could inform efforts aimed at reducing their proliferation. Therefore, I believe that the questions being investigated in this manuscript are highly relevant and that the data presented could provide a valuable contribution to the literature. However, I have several reservations with the manuscript in its current

form, which is why I advise a revision of the paper. Below I outline a set of major concerns to be addressed before suggesting the publication of this paper and a set of minor concerns and comments, which I do not believe to be essential, but which may help improve the manuscript. I hope my comments help the authors improve their manuscript.

Hereafter is a summary of major concerns I have:

Theoretical background:

1. The theoretical background for the “miserly hypothesis” (lines 36-44) could be more elaborate. More specifically, the authors could go beyond mentioning correlations between misbeliefs and analytic thinking styles by providing a brief summary of how these thinking styles affect the formation of (mis)beliefs and reasoning about evidence. Furthermore, it is not acknowledged that evidence for correlations between analytic thinking and misbeliefs does not imply that misbelievers tend to reason *less*, but that they reason less analytically and more intuitively (i.e., *differently*). While some literature indeed suggests that *more* reasoning reduces susceptibility to misbeliefs (e.g., Bago et al., 2020 cited by the authors in the discussion), much of the literature on the relationship between thinking styles and misbeliefs actually focuses on how reasoning differently (e.g., focusing more on accuracy, Pennycook et al., 2021, *Nature*) reduces this susceptibility. This is also the case for some of the literature cited by the authors in support of the claim that individuals may reason *less* (e.g., the CRT assesses the extent to which individuals reason analytically rather than intuitively, instead of assessing the absolute extent of any reasoning). Hence, framing the “miserly hypothesis” to imply that misbelievers reason *less* based on extant literature on correlations between analytic thinking and misbeliefs is not entirely warranted. For these reasons, I believe that elaborating in more detail on the theoretical background of the “miserly hypothesis” and clarifying whether the postulated reasoning style under this hypothesis merely entails *less* reasoning overall or less *analytic* reasoning would be important to situate this hypothesis in the extant literature.
2. Similarly, the “information preference hypothesis (lines 45-48) is not clearly defined and the line of argumentation for this hypothesis would require more attention and elaboration, given its centrality to the manuscript. The theoretical background for this hypothesis could benefit from a clear line of argumentation for why individuals holding misbeliefs may reason differently than those not holding such beliefs and what exactly these differences in reasoning entail. For instance, the authors could develop a theoretical argument for this hypothesis based on the body of extant studies showing that susceptibility to misbeliefs is related to a lack of intellectual humility (which could influence reliance on self-generated information), or excess distrust of authorities (which could influence reliance on traditional notions of expertise). Providing more extensive, theory-based reasoning for this hypothesis could make the distinction to the “miserly reasoning” hypothesis (which, as argued

above, could also be interpreted in terms of individuals reasoning *differently* rather than *less*) clearer, thereby highlighting the contribution of the paper.

3. The authors claim to examine how individuals reason about evidence, while most of the information presented in the study materials relates to the sources providing evidence, rather than evidence itself. This creates several issues, which I outline below:
 - This seems to be part of nature of the ExPEX framework the authors apply. This framework for creating study materials appears to be suitable for investigating jurors' reasoning about expert opinion (for which the framework appears to have been introduced, as outlined in Martire et al., 2020, cited by the authors). However, the authors compare their findings to those of studies investigating susceptibility to misinformation (e.g., fake news) and conspiracy theories and imply that their findings allow for inferences about these phenomena, creating a discrepancy between the phenomenon that appears to be investigated and the phenomena about which the authors make claims. The authors could address this discrepancy by either providing a theoretical argument in the introduction for why this generalisation is valid or by mentioning this as a limitation to the generalisability of results in the discussion.
 - A large body of research in the fields of persuasion and misinformation (although the evidence from the latter is less consistent) has shown that substantive information ("evidence") and information about the sources presenting this information can be processed (i.e., reasoned about) in different ways under different circumstances. Therefore, a strong theoretical argument would be needed to justify drawing inferences from the presented studies about how individuals reason about evidence (rather than about information about the sources presenting evidence). For instance, such an argument could be developed based on Kruglanski & Thompson's unimodel, which suggests that there should not be qualitative differences in individuals' evaluation of substantive content and information about its source when their structural features are held constant.

Methods & (reporting of) results:

1. The authors conduct parametric tests (t-tests, ANOVA) on count data (i.e., word counts when comparing the length of justifications or the counts of different justifications that are provided). Parametric tests are not suitable for analysing count data, as count data usually violate distributional assumptions of these tests, potentially leading to inaccurate results. This issue could be resolved by either demonstrating that the data do not violate the assumptions imposed by the tests the authors employ or, if that is not possible (e.g., because the data do violate these assumptions), instead applying statistical tests that are suitable for count data (e.g., Chi-squared tests). Additionally, even if assumptions are met, there may be issues with applying ANOVA to

data from studies in which factors are unbalanced, which may be the case here due to coding (rather than manipulating) the different types of justifications. Therefore, if assumptions for parametric tests are met, it would be good if the authors provided a justification for why ANOVA (and the particular type of computing sums of squares they employed) is suitable for their data, or if they applied a method that is more suitable for such data. If the data is balanced, this comment can be of course disregarded. As analysis choices like these can substantially impact the nature of results, I strongly encourage these issues to be dealt with in detail.

2. The authors indicate larger sample sizes in describing the final samples after exclusions (lines 121 and 127) than in the section titled “qualitative coding”. Also, the degrees of freedom of the reported t-tests suggest that the lower sample sizes were used for quantitative analyses. Executing the openly available analysis code for both studies also suggests that analyses were conducted on the smaller of the two reported sample sizes per study. This reporting error should be corrected and any exclusion procedures, as well as which data (including or excluding which participants) were used for which analyses should be reported.
3. When reporting the interaction results, authors state that “follow-up Bonferroni corrected pairwise comparisons revealed no differences between the groups in either the number of self-generated or Present-irrelevant justifications in both studies” (lines 253-255). However, in the abstract, they state that “compared to mainstream believers, fringe believers relied relatively more on less relevant or self-generated information than on information relevant to expertise”. None of the analysis reported in the paper seem to support the claim in the abstract. This could be resolved by either reporting analyses in support of this claim or omitting the claim.

Below I outline several minor concerns as well as other comments that I do not see as crucial for the revision of the manuscript but that may help improve it:

Theoretical background:

1. The authors focus in their discussion of the focal phenomenon on how individuals with different reasoning styles come to develop misbeliefs (i.e., the implied causal order is that reasoning styles affect misbeliefs). However, because individuals who already hold misbeliefs are studied, it may well be that holding misbeliefs may change how individuals reason about information they are exposed to after having formed these beliefs. For example, Nera (2024; *Zeitschrift für Psychologie*) argued that the relationship between conspiracy mentality and belief in (specific) conspiracy theories may be bidirectional. A similar point could be made here, particularly as the measures of misbeliefs employed in the studies resemble measures of belief in conspiracy theories
2. The authors cite Swami et al. (2014) in line 38 and highlight in the same paragraph that analytic thinking is *consistently* shown to negatively correlate

with misbeliefs. However, Većkalov et al. (2024; *JESP*) failed to replicate important findings by Swami et al. (2014) showing causal evidence for these effects. This could be mentioned to provide a more nuanced overview of the literature.

Research design:

1. The procedure for categorising participants based on their beliefs is mentioned first in lines 105 and 106, stating that participants were pre-screened based on the BEIC protocol two months before the study. However, in lines 162 to 165, it is mentioned that the BEIC protocol was administered at the end of the study. This should be corrected or, in the case of separate measurements, clarified.

Method:

1. In the qualitative coding section, authors state that 3 different types of justifications were coded, but only an example of one is provided. It would be insightful to provide examples of the other types of justifications as well.
2. In the results section, authors mention an “exploratory” set of analyses, which could be read to imply that other analyses are confirmatory. However, no specific analyses of the qualitative data are mentioned in the preregistrations (except for mentioning that quantitative content analyses will be conducted), making them exploratory in nature. It would be good to specify the nature of analyses (i.e., exploratory or confirmatory).

Discussion:

1. Given the potential relevance of the presented findings for understanding and potentially reducing the spread of misbeliefs, I think a brief discussion of the consequences of beliefs that are justified/evaluated through the reasoning processes that are investigated would be interesting. For instance, prior research has shown that individuals’ belief in specific conspiracy theories has stronger effects on their behavioural intentions for individuals who evaluate their own (conspiracy) beliefs more strongly than those who do so to a weaker extent (Mang et al., 2024, *Personality and Individual Differences*). Given that different reasoning processes can result in different behavioural outcomes, I believe a discussion of the potential consequences of the differences in reasoning between individuals holding misbeliefs and individuals who do not would highlight the practical relevance of the presented research.

Other comments:

1. Since some of the beliefs based on which individuals were categorised are very similar to measures of conspiracy theories, labelling individuals holding these beliefs “conspiracy believers” or “misbelievers” may be a more precise description than “fringe believers” and would help readers locate the contribution of this paper within the literature on misbeliefs.

2. The manuscript is fully understandable but contains a few minor grammatical and other errors. I would have the manuscript checked for these errors.

While I have mostly focused on things that can be improved, I also want to highlight that I believe the authors have conducted highly relevant and interesting research and written it up in a very concise and understandable way (I particularly liked the discussion, which was very clear). Furthermore, the authors have made their data and reproducible analysis code openly available, which I find laudable.

I wish the authors all the best for the revision of the manuscript.

Round 2

Dear Authors,

Thank you for submitting your revised manuscript for consideration in our special issue. We all agree that the revised version has substantially improved. For instance, the analytical strategy is now more appropriate for the data, and the introduction is more elaborative in laying out the background and motivation for your research. Thank you for the efforts that went into these comprehensive changes. We still believe there is some room for further clarification and improvement. I will not reiterate the Reviewers' comments as they were very detailed and structured in their feedback, but I ask you to respond to each of their points and outline how you addressed them (or provide a well-founded rebuttal) in a revision letter.

In addition, I would like to request the following changes. Some of these may overlap with the Reviewers' comments:

1. Please add sample sizes (Ns) for both studies to the abstract.
2. Thank you for improving the description of non-significant effects. However, you still seem to predict a null-effect: "we expect Fringe believers and Mainstream believers not to differ in the number of justifications." For this, you would need a direct test. Alternatively, write "not to differ significantly."
3. There are still some typos, missing punctuation, and words (sometimes critically changing the meaning of the text, e.g., line 349), and similar issues. Please thoroughly address these.
4. Capitalize headings in line with APA 7.
5. Please italicize all statistical letters, except Greek ones, in line with APA 7. There are some inconsistencies.

6. Please address all inconsistencies identified in the citations report that you can find here: <https://drive.google.com/file/d/18YqiswG1AMoHKMCITGwXk5wWI9P3t-2W/view?usp=sharing>
7. In the figure, please make the data points smaller and jitter them.
8. Explain what correction was used for multiple tests in the subcategory analyses. I would suggest using Holm correction.
9. In the subcategory prevalence tests, would it make sense to also provide a merged analysis (combining data from Studies 1 and 2) to increase power?
10. I did not understand why responses used to generate the coding book were not analyzed (also see Reviewer). Coding them would be a common standard and increase power.
11. Some of your evidence supports the miserly hypothesis, and this should be more reflected in the interpretation and reporting (including in the abstract). I think you find mixed evidence for both hypotheses, which has implications for interventions.

Thank you again for submitting this substantially improved manuscript to our special issue. We are getting much closer to reaching the threshold that warrants publication. I would like to receive a revised manuscript within one month, by August 31. Please do not hesitate to reach out to me if you need additional time or clarifications.

Best,

Jonas R. Kunst
Editor-in-Chief

Reviewer 1:

Thank you for the opportunity to review this substantially improved manuscript. I have re-read the paper, and again I do think that the study is interesting (and I agree with the authors entirely on the need to study the role of epistemology). However, I'm still struggling a bit with regards to the design/analyses and the conclusions being drawn.

First, from the R code, I'm just wondering why were interaction terms between belief (regardless of whether it is treated in a dichotomous manner or on a continuous scale) and condition (high vs. low quality report) not included in any of the models? Maybe I missed something, but this was not clear to me from the main text and figures. Also, the authors have now clarified that belief was measured at the end of the study, following the evidence evaluation task. I wonder if this is potentially

problematic, as the analyses are then conditioned on a post-treatment variable, which could affect results in unpredictable directions (see, e.g., <https://doi.org/10.1111/ajps.12357>).

Third, my primary concern was/is not about self-report measures or demand characteristics per se, but rather about whether the study provides a sufficient test of the stated hypotheses. I'd therefore still prefer if the authors continue to tone down the implications that their analyses are necessarily inconsistent with the miserly hypothesis (as represented within the literature)—the discussion for the information preference hypothesis is much more justified, in my view—and add some further nuances. I will illustrate this with some examples. The two hypotheses in this study are, essentially, whether cognitive miserliness causes implausible beliefs or information preference causes implausible beliefs. However, instead of assessing or varying cognitive miserliness and/or information preference as the independent variable, the paper is taking a dichotomized categorization of the outcome of the hypothesized processes, belief in implausible beliefs (vaccines, climate change, moon landing, and flat earth), to work backwards to infer if either cognitive miserliness or information preference is present in responses to open-ended self justifications following an evidence evaluation task. In this way, the paper's approach is, at least to me, similar to categorizing people with blood cancer, colon cancer, and lung cancer into a cancer group, to contrast with people without any cancer in a mainstream group. Working backwards whilst taking a broad definition, the approach can perhaps lead to the conclusion that smoking isn't a cause of cancer, when on average the cancer and mainstream groups do not differ on their levels of smoking. This applies even assuming that the smoking levels (or cognitive miserliness) are measured well, and even though all these different types of cancer fall under the broad definition of cancer (and all the claims fall under the broad definition of implausible claims). Unless all cancer is theorized to be caused by smoking (or all belief in implausible claims are theorized to be caused by cognitive miserliness), the approach does not actually provide a sufficient test of the smoking-cancer hypothesis. I think the authors do acknowledge this in the revised version, which I really appreciate. In particular, in lines 151-153, the authors now state "..., any difference in miserliness, if it is a cognitive style, should apply even in these seemingly unrelated contexts", and in line 465, the authors now state "..., a lazy cognitive style, if it is a general style, should present itself in all contexts". However, and I may be wrong, I really do not think that that's a claim people are seriously making in the literature—hence why I still see the miserly hypothesis that's being tested within this study as the "extreme" version. Even in Pennycook et al. (2019) that the authors cite as example, the actual discussion in the paper is nuanced, with analytical thinking style considered on a spectrum and having different effects depending on the context, e.g., "How, then, do we make sense of the seemingly contradictory finding that analytic thinking exacerbates motivated reasoning in terms of attitudes about topics such as global warming (Kahan et al., 2012), but not perceived accuracy of fake news? One possibility is that exceptions to analytic thinking's general tendency to protect against epistemically suspect beliefs occur in cases where analytic thinking is unlikely to be successful. That is, even the most reflective members of the general population cannot overcome their lack of training

and knowledge in the realm of, for example, climate science. Thinking analytically about extremely complex scientific issues is not likely to have a directional effect because the factors that climate scientists think about when judging the likelihood of anthropogenic global warming will not be the same as the factors that lay people think about.”

Minor:

- Line 165 “Condition; High vs. Low” is a bit unclear in the first instance. Maybe change to “high-quality vs. low quality report”?
- line 250 “study 2” should be “Study 2”

Reviewer 2:

I enjoyed reading the revised version of the paper titled “Do those who believe misinformation reason lazily or differently? A quantitative content analysis”. It is apparent that the authors substantially revised their manuscript to address the points raised in the first round of reviews. I believe that these revisions, particularly the reporting of more appropriate analyses as well as clarifications of some conceptual and methodological aspects that were previously unclear/underdeveloped, substantially improved the manuscript. However, there are still several issues that I recommend being addressed before suggesting the publication of this paper. Below I summarise these points. Some of these points pertain to my previous comments, and others to issues addressed by the other reviewer and the editor or to issues that have arisen after the revisions the authors implemented.

Conceptual issues:

1. Definition of fringe believers: I appreciate that the authors responded to my previous comment about their definition of what constitutes a fringe believer. However, I am not entirely convinced that their definition of a fringe belief is indeed distinct from belief in specific conspiracy theories. At least some of the beliefs that are used to categorise individuals as fringe or mainstream believers are clearly conspiratorial in nature and similar items are commonly used when measuring (propensities to hold) conspiracy beliefs (e.g., “vaccines are harmful, and this fact is covered up”, “the Apollo moon landings never happened and were staged in a Hollywood film studio”). This also contrasts the authors’ definition of implausible claims on pages 5 and 6, which explicitly excludes conspiracy beliefs from the beliefs in implausible claims the authors claim to investigate. Furthermore, the revised and more detailed summary of the information preference hypothesis describes a way of reasoning about evidence that is very similar to that often described in the literature on conspiracy beliefs (e.g., low reliance on/rejection of mainstream/expert evidence). Overall, I see substantial overlap between the beliefs in implausible claims as currently conceptualised by the authors and belief in conspiracy theories. While I understand the authors’ reasons for not

labelling participants as “conspiracy” believers, clarifying how these two types of beliefs are related to, or different from, one another, would be helpful.

2. Cognitive styles: The authors refer to “cognitive styles” and cite studies that, to my knowledge, do not primarily focus on relatively stable cognitive styles, but rather on how inducing a situational focus on accuracy can mitigate false beliefs (e.g., page 3, lines 54 and 55: “*Cognitive styles beyond the intuitive vs analytic dichotomy, like focussing on accuracy, are also related to false beliefs (Pennycook et al., 2021)*”). To me, the term “cognitive styles” has a connotation of stable individual differences in cognition, which is why I believe citing studies that focus on how individuals can be *situationally* encouraged to think more critically to support claims about cognitive styles is not ideal in these cases. Instead, I would cite literature that actually examines more trait-like cognitive styles. This is just a minor technicality though.

Methodological issues:

1. Categorisation of participants based on beliefs: It is still somewhat unclear to me why participants were categorised based on their agreement with a single statement rather than using the continuous belief measures as focal predictors. The authors refer to previous work to justify this decision and I understand that the authors want to be consistent with this previous work and with their preregistration, but I think a more detailed justification of this decision in the current paper would be helpful for readers.
2. Responses for piloting codebooks: It would be good to explain why responses used for piloting the codebooks were not used for the reported analyses.

Reporting & interpretation:

1. Preregistration of analyses: I appreciate that the authors now transparently state that they preregistered hypotheses, but not specific analyses, but it could be additionally mentioned that this makes the reported analyses exploratory in nature.
2. Power analysis: When reporting the power analysis (page 9, lines 199-201), please report the program/package that was used and provide all information that is needed to reproduce the analysis. Also, it is unclear to me what makes an effect size of $f = .25$ “practically significant”. This should either be elaborated on or omitted.
3. Reporting of sample demographics on pages 9 and 10: It would be more informative to report the percentages of participants identifying with/belonging to *all* groups rather than only reporting the percentages of one group per variable (e.g., only reporting “55.7% identified as male/man”; page 9, line 210).
4. Plots on page 17: As pointed out by the editor, 95% error bars would be helpful. While I understand the argument by the authors that displaying the full distribution of data can be helpful, this could also be done while at the same time providing 95% error bars (e.g., by plotting individual data points or

providing a density plot). I personally find density plots (e.g., violin plots or similar ones) a bit more intuitive for visualising the distribution of data (which appears to be the authors' goal) than plotting individual data points (although they are also not without limitations). When plotting individual data points, adding more random variation to the location of points (i.e., "jitter") and making points smaller than is currently the case would make it easier to get an overview of the distribution of data points, as the current visualisation (with large points that overlap quite a bit) is a bit cluttered and makes it difficult to gauge the density of data points at any given value.

5. Statistical significance of differences between justification categories: I would be interested in an interpretation of why there are statistically significant differences in the number of justifications between fringe and mainstream believers for the umbrella category "present-relevant" justifications, but no statistically significant differences between any of the subcategories in this umbrella category. Do you think this might have to do with statistical power or with the use of different analytical approaches or something else?
6. Effects of the quality manipulation: The authors state that they did not find "consistent or convincing evidence for an effect of quality" (page 20, line 429), while reporting two statistically significant effects of quality. I understand that the authors are reluctant to interpret these effects in substantive terms, as the effects of quality condition contradict each other across studies, but it is unclear why the individual effects are any less convincing than other statistically significant effects reported. Given that there were substantial design differences between studies, there are various potential causes for differences in effects. If at all possible, I recommend that these findings should also (briefly) be interpreted.
7. Discussion of the miserly hypothesis: The authors state that their "findings are inconsistent with the Miserly Hypothesis", but later on acknowledge that fringe believers wrote shorter responses in Study 1 (but not Study 2; page 21, lines 433-434). This could actually be interpreted to suggest that there is some (albeit inconsistent) evidence for the miserly hypothesis, which should be acknowledged. Furthermore, the fact that fringe believers provided fewer relevant justifications, but at the same time did not provide more irrelevant justifications could also be interpreted as a manifestation of (a variant of) the miserly hypothesis. The authors conclude that the fact that fringe believers rely less on relevant evidence (while not relying more on irrelevant evidence) supports the idea that fringe believers hold "preferences for different types of evidence", although this is not clear from the data: the results merely show that they rely less on the same type of evidence (i.e., relevant evidence) that is relied on most often by participants, not that they rely on different types of evidence (but on the same type of evidence to a different extent). In other words, while the data is also not entirely consistent with the miserly hypothesis (which makes predictions about the quantity of reasoning), the information preference hypothesis is also not fully supported (as it makes predictions about fringe and mainstream believers relying on qualitatively

different types of evidence), and it could be argued that what the authors observed in their studies is somewhere in between these two hypotheses: fringe believers relied quantitatively less on the most prevalent type of evidence, but did not rely more strongly on other types of evidence (i.e., fringe believers might not be miserly overall, but only when it comes to reasoning about relevant evidence). A more nuanced discussion of the findings with regards to these points might be good. Relatedly, instead of primarily recommending to encourage educating people about the quality of evidence rather than encouraging them to deliberate more (which is how I interpreted the authors' practical recommendations), combining both appears to be a reasonable approach as well (i.e., encouraging more deliberation of high-quality evidence).

8. Practical recommendations: On page 23, line 499, the authors wrote "*Some scholars suggest that false beliefs can be 'corrected' by encouraging more deliberation (Bago et al., 2020) or prompting more analysis (Swami et al., 2014).*". Particularly when it comes to practical recommendations, I would not cite studies that have failed to replicate (i.e., Swami et al., 2014).
9. Supplementary analyses: The authors state that they used the full data set for each study for the supplementary analyses. This is briefly explained in the annotated code (i.e., no participants need to be excluded when using belief as continuous measures), but not in the paper (which should be corrected) and it should somewhere be reported whether the other exclusion criteria, such as attention checks, were applied for these analyses as well. Furthermore, the authors only report the statistical output without providing any additional information. It would be good 1) to report the analyses either in properly annotated tables or in full text as you would report analyses in the main text and 2) to highlight similarities/differences in results between the analyses reported in the main text and those reported in the supplementary material somewhere in the manuscript or supplementary material.

Code:

1. It is laudable that the authors provide well-annotated code that allow to reproduce the analyses reported in the paper. One minor comment I have is that many analyses that are not reported in the paper are included in the scripts that are provided without explicitly mentioning which analyses correspond to the ones reported in the paper, which could be confusing for individuals who want to reproduce the analyses. Given that it was still possible for me to reproduce the reported analyses, I do not ask the authors to change this, but just wanted to note this as a suggestion for future work.

Other:

1. There are several style and grammar errors throughout the manuscript. Below is a list of some errors I spotted:

1. Page 7, lines 151-152 should either say “about which participants reasoned” or “the facts participants reasoned about” instead of “the facts about which participants reasoned about”
2. Page 7, line 165: “between groups design” is missing a hyphen (it should say “between-groups design”)
3. Page 16, line 351 is missing a full stop
4. Page 20, line 421 should say either “participants read a report” or “participants read reports”, not “participants read report”
5. Page 22, line 455: *also* or *either* can be omitted from “also not without limitations either”,
6. There are some capitalisation errors: e.g., lines 540-541: “Journal of Economic perspectives”

Even though I think the paper is already highly interesting, I believe that some further refining of the aforementioned details would be needed to realise the full potential of this paper. I wish the authors all the best for the next steps with this manuscript.